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THE MEASURABILITY OF ATTENTION BY PROFESSOR WIRTH'S METHODS

By L. R. GRISSLER

In a recent number of the *Psychologische Studien*, Professor Wirth publishes an article entitled "Zur Messung der Klarheitsgrade der Bewusstseinsinhalte,"¹ which is a reply to my "Critique of Professor Wirth's Methods of Measurement of Attention."² The Reply consists of twelve sections, consecutively numbered; and the reader naturally infers that it presents twelve different objections to my Critique. As a matter of fact, the contents of the sections are not sharply differentiated, and the argument of one is sometimes repeated in others. Hence I shall not attempt to take them up in order, but shall merely refer in parenthesis to the particular section in which the point under discussion receives the greatest emphasis. I shall, furthermore, confine myself to the four articles mentioned in my previous paper.³ Wirth's book, "Die experimentelle Analyse der Bewusstseinsphänomene" (§ 1), arrived here after my manuscript had gone to print, and I was, therefore, unable to reply to it,—although this explanation may seem inadequate to my opponent, who devotes more than a page to my work on "The Measurement of Attention" some half year before its appearance⁴ (§ 6 and § 10). I do not deny that Wirth's book clears up some of the obscurities here mentioned, just as his Reply is in some respects an illuminating and welcome commentary upon his own work; but these facts do not affect the validity of my original criticism. I am sure, too, that my article contains nothing to justify the personal attacks scattered throughout Wirth's pages. The charges are not only absolutely untrue to fact, but they are also unsuitable for public discussion and refutation. I shall make no reference to them in what follows.

The keynote of Wirth's article is the complaint that he has

¹ *Psych. Stud.*, V, 1909, 48-72.

² *AM. JOUR. OF PSYCH.*, XX, 1909, 120-130.

³ Zur Theorie des Bewusstseinsumfanges und seiner Messung, *Phil. Stud.*, XX, 1902, 487-669; Die Klarheitsgrade der Regionen des Sehfeldes bei verschiedenen Verteilungen der Aufmerksamkeit, *Psych. Stud.*, II, 1906, 30-88; Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reaktionsversuchen, A. Kästner and W. Wirth, *Psych. Stud.*, III, 1907, 361-392; IV, 1908, 139-200.

⁴ *AM. JOUR. OF PSYCH.*, XX, Oct., 1909, 473-529.

been entirely misunderstood. The possibility of misunderstanding I frankly admitted; Wirth's language, I said, is "both difficult and obscure," and is "not always consistent;" and indeed, any one can verify the point for himself by trying to translate some page of Wirth's writing into another language. I, therefore, offered my "interpretation of Wirth's attitude . . . with all reserve," and I am now merely repeating a former statement when I say that I was in constant doubt as to the very nature of his main problem (§§ 1-5). Nevertheless, in my discussion of Wirth's results, I tried to keep the two possible forms of his problem equally in mind, and to remember that his intention might be to measure either the range of consciousness or the range of attention only. Hence Wirth's principal accusation, that I have simply and completely misunderstood him, is incorrect. I may complain, on my side, that in some points at least he has misunderstood me, and has interpreted my remarks in such a way as to exaggerate, or even to create, difficulties which did not exist either in my words or in my thought. Moreover, he often criticises me, without reason, rather for what I have not said than for what I did say.

Thus, with an experimental arrangement as complicated and a method as elaborate as his, it was impossible for me to do justice to all details in a short discussion. For I did not, as Wirth puts it at the beginning of his Reply, intend my article to be "eine Orientierung über den gegenwärtigen Stand der experimentellen Analyse der Aufmerksamkeit."¹ I did not even offer an exhaustive review of Wirth's own work, but merely "a discussion of his contributions to the subject."² Again, Wirth's interpretation of my sentence: "thus the results confirm and extend those of previous tachistoscopic experiments upon the range of attention," implies that I had written: "of *his* previous experiments, etc." (§ 5).³ His statement that I had failed to see the logical relation of the "entweder—oder" (§ 5) in Wundt's discussion of Wirth's tachistoscopes, I can only characterize as amazing. As a matter of fact, I had never even hinted at a reference to that particular phrase. I simply mentioned Wundt's belief that the tachistoscope may be used for measuring not only the range of attention, but also the "Gesamttumfang des Bewusstseins."⁴

¹ *Psych. Stud.*, V, 48.

² *AM. JOUR. OF PSYCH.*, XX, 120.

³ *Ibid.*, 57. For the same reason his objection to the word "extend" falls to the ground. Another obvious case of misunderstanding is the Footnote, p. 61 of his Reply (§ 8).

⁴ *Physiol. Psych.*, III, 1903, 358 f. "Je nach der Anwendung dieser

But the climax of Wirth's misrepresentation is found in his objections to my discussion of his reaction-experiments (§ 12). I did not, it is true, mention the fact that their purpose was purely negative; that is, that Wirth meant to compare reaction-times (which have frequently been assumed to express variations of attention) with the results obtained by his previous method (the *Schwellenmethode*), in order to show that they do not afford as reliable a measurement of attention as do his liminal clearness-values. But then he himself makes no such explicit statement, either in the form of a problem or in that of a conclusion. The only passages which hint at any purpose, with regard to the reaction-experiments, are the following: "da es uns vor allem darauf ankam, die Resultate auf die früheren Klarheitsmessungen nach der Schwellenmethode zu beziehen," etc.;¹ and "wenn irgendwo, so scheint nun gerade für die Abhängigkeit der Reaktionszeit vom Klarheitsgrade der Motivauffassung, *die wir hier im einzelnen untersuchen wollen*," etc.² From these two incidental passages the reader is left to conclude for himself what the "entscheidende Hauptpunkt" of the investigation is; namely, as Wirth now says in his reply, but had not said before, "dass die Methode der (sensoriellen) Reaktion an der Hand der unter vergleichbaren Bedingungen gewonnenen Resultate nach der *Schwellenmethode*, *die allein für uns als Klarheitsmass gilt*, daraufhin kontrolliert werden sollte, wie weit sich die Klarheitsverhältnisse auch in dem Zeitwert widerspiegeln."³ This implication was not observed by me, nor seemingly by A. A. Grünbaum,⁴ who lately published a more elaborate review of Kästner and Wirth's reaction-experiments. For Grünbaum the problem of the investigation is simply to find out "wie das Klarheitsrelief, das durch verschiedenartige Verteilung der Aufmerksamkeit erreicht wird, sich in den Reaktionszeiten widerspiegelt." There is here no mention of a proposed comparison with the results of the *Schwellenmethode*. Nor does this problem occur to Grünbaum when he actually compares the results obtained by the two methods. He merely says: "man sieht leicht,

verschiedenen an dem Apparat möglichen Versuchsweisen kann man denselben entweder, ähnlich wie das Falltachistoskop Fig. 339, zu Bestimmungen des Umfangs der Aufmerksamkeit, oder aber auch zu Versuchen über den Gesamtumfang des Bewusstseins verwenden," etc. "Ein zweiter Apparat, mit dem in einer von der soeben beschriebenen etwas abweichenden Weise sowohl Umfangsbestimmungen der Aufmerksamkeit durch momentane Apperceptionsversuche, *wie solche des Gesamtbewusstseins* . . . ausgeführt werden können," etc. (Italics are mine.)

¹ *Psych. Stud.*, III, 387.

² *Ibid.*, 390. (Italics are mine.)

³ *Psych. Stud.*, V, 69.

⁴ *Zeits. f. Psych.*, LIII, 1909, 97-102.

dass beide Reihen nicht annähernd parallel verlaufen,—was nach der Verschiedenheit der psychologischen Bedingungen beider Methoden auch zu erwarten war, wodurch aber auch die Möglichkeit einer objektiven Wiedergabe des Klarheitsreliefs nach beiden Methoden in Frage gestellt wird.”¹ I confess, too, that I do not see why, if Wirth objects to the assumption that his reaction-experiments were meant to be “eine Methode zur Messung der Klarheitsgrade,”² he should compare their results with those of his *Schwellenmethode*. And why does he speak of an “Übereinstimmung der beiderseitigen Aufmerksamkeitsmasse?”³ Surely, such language is both inconsistent and misleading.

Aside from these more technical issues, there remain certain important points concerning which a real disagreement seems to exist between Wirth and myself. In the first place, I cannot follow Wirth in drawing a sharp distinction between “the activity of attention as the most important subjective condition of a certain formation of the clearness relieve” and “the actually attained degree of clearness” (§ 3).⁴ I myself never experience anything like an ‘activity,’ which might be supposed to be under voluntary control, nor have I ever met with it in the introspections of my observers; and the psychologists who occasionally mention it in experimental contexts, do not agree as to its nature. Hence the meaning of the phrase is not as unequivocal as Wirth assumes it to be.⁵ For my own part, I must continue to identify the ‘activity of attention’ with the attained degree of clearness.

Another point upon which I disagree with Wirth concerns the general nature of the attentive consciousness, especially in the tachistoscopic experiments. I happen to have taken part lately in over one thousand monocular tachistoscopic observations on liminal and slightly supraliminal brightness-

¹ *Ibid.*, 99.

² *Psych. Stud.*, V, 69.

³ *Psych. Stud.*, IV, 156.

⁴ *Psych. Stud.*, V, 52.

⁵ Perhaps Wirth's distinction is meant to be identical with Wundt's distinction between *Aufmerksamkeit* and *Apperception*, as it is presented in the *Physiol. Psych.*, III, especially 341: “Nach allem diesem sind Aufmerksamkeit und Apperception Ausdrücke für einen und denselben psychologischen Thatbestand. Den ersten dieser Ausdrücke wählen wir vorzugsweise, um die subjective Seite dieses Thatbestandes, die begleitenden Gefühle und Empfindungen, zu bezeichnen; mit dem zweiten deuten wir hauptsächlich die objectiven Erfolge, die Veränderungen in der Beschaffenheit der Bewusstseinsinhalte an.” Wirth's “activity of attention” would then be identical with Wundt's “Thätigkeitsgefühl.” But it is well known that this side of Wundt's apperception theory has been sharply criticised, and has found little acceptance.

differences which, placed about 12cm to the left of a constant fixation point, were exposed for one hundredth of a second and judged under three different distributions of attention. The conditions were very similar to Wirth's, although, of course, they were far less complicated. In these observations, my attention was for the moment raised to the very highest pitch of concentration, while the background was so obscure that I could not possibly analyze it. I have found certain observers who agree with me in experiencing, under these conditions, a marked difference between maximal and minimal clearness-levels, while others distinguish simultaneous processes of intermediate clearness-degrees. This difference, which is borne out by the results of a large number of systematic experiments on degrees of clearness,¹ has led me to assume two different types of the attentive consciousness, the 'dual-division' and the 'multi-level' formation. Evidently Wirth, and probably also Wundt, belong to the latter type. Whether Wirth's results would have been different if he had had observers of the first type, it is impossible to say. But it still remains a disadvantage of his experimental work that in all of the final series he was the sole observer (§ 10). The same objection is implied in Grünbaum's review, although he refrains from making it an explicit criticism.² It is true that Wirth made preliminary experiments with several less skillful observers; and he regards the results as utilizable, because they show a rough agreement with some of his own observations obtained at an early stage of practice. Nevertheless, these other observers made binocular, and not monocular observations upon a plane, and not upon a funnel-shaped field of vision; so that the experimental conditions in their case were quite different from (much simpler and easier than) those under which Wirth himself later completed the final series. I might add that Grünbaum also considers Wirth's experimental conditions as too complicated and artificial, and the requirements made upon the observer's attention and introspective self-control as too difficult (if not even self-contradictory), to guarantee a successful solution of the problem.

Finally, Wirth objects to my using collectively the results of his *Schwellenmethode* (§ 11). I combined into a single frequency curve all the numerical clearness values given in his six schemata, each schema representing a different distribution of attention. The reason for his objection is that some of the schemata were obtained at an earlier, some at a later stage of

¹ The Measurement of Attention, AM. JOUR. OF PSYCH., XX, 1909, 473-529.

² *Op. cit.*, 98.

practice in observation. Nevertheless, in his own final conclusions, Wirth neglects this practice-effect altogether, and directly compares with one another the results expressed in the various schemata. Besides, I had, for my own satisfaction, constructed individual frequency curves for all the different distributions of attention, and had found great similarity among them; so that for my particular purpose I saw no further necessity of separate treatment. However, even if the different stages of practice had influenced the results, I might justly have followed Wirth's example in his application of the principle of analogy, and might have assumed that longer practice would probably eliminate large irregularities in the frequency distribution.

In conclusion, I still maintain that Wirth has failed to solve his problem, not because, as he tries to show, I had been mistaken in its nature, but because, as I have said before, his numerical clearness values were "obscured or invalidated by complicating factors," and because "the restricting conditions of his experimental arrangement" served only to enhance "the impossibility of overcoming difficulties of observation" whose existence he himself has frequently admitted.